above named have all but provoked, "imagine a locust stridulating in the centre of a mass of iron one mile in all directions" (*sic*). The idea is charming, countrified, bucolic, but perhaps rather cold for the poor insect! "It is admitted he could be heard, and about sixteen times quicker than in the air. . . ." (the steps of this grand calculation must perforce be omitted). "The mass of iron thus displaced" (*i.e.* by said locust) "would weigh not less than 729,749,050,612 tons, and would be so moved by the strength of the locust."

The thought is too tremendous! so, locust-like, I must cease to "stridulate," lest I bring down the solar system in ruins on the heads of innocent humanity.

W. H. STONE

Kryptogamen-Flora von Schlesien. Vol. III.: *Pilze.* Bearbeitet von Dr. J. Schröter. (Breslau: J. U. Kern.)

DR. COHN'S "Cryptogamic Flora" is already so favourably known by the portions which have appeared, that the announcement of any subsequent part is sure to be received with satisfaction. The first part of the Fungi, by Dr. J. Schröter, is just issued, and consists almost entirely of an introduction of nearly 100 pages, carefully digested and summarised, concluding with the order of classification adopted. The three groups or primary divisions are:—(1) Myxomycetes; (2) Schizomycetes; and (3) Eumycetes. The latter embraces the Chytridiei, Zygomycetes, Oomycetes, Protomycetes, Ustilaginei, Uredinei, Auriculariei, Basidiomycetes, and Ascomycetes, with an appendix for the incomplete Hyphomycetes, Tuberculariei, and Sphæropsidei. As the present part contains only a portion of the Myxomycetes, no opinion can be formed of the manner in which the foregoing skeleton will be filled up; but, as this portion is based upon Rostafinski's monograph, no exception can be taken thereto. The real difficulty lies further ahead, and whether the knot is to be untied or cut cannot be predicted.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Terminology of the Mathematical Theory of Elasticity

I HAVE been greatly interested by the letters on this subject from Prof. K. Pearson (NATURE, vol. xxxi. p. 456) and Prof. A. B. W. Kennedy (vol. xxxi. p. 504), and I have looked forward with pleasure to further communications from other eminent "elasticians." As, however, no better qualified person seems disposed to continue the correspondence, and as I am practically interested in a definite settlement of elastic terminology, I venture to offer a few remarks on the subject.

(1) Nothing could be better than Prof. Pearson's term *state of ease* for the condition of an elastic body when capable of enduring a certain amount of stress, without showing permanent set on its removal. This is worthy of Clifford, and is sure to make its way.

Prof. Kennedy has extended this term by applying "maximum state of ease" to the condition in which the body may be strained to its elastic limit without set. Perhaps *ultimate state of ease* would be more significant, and *limited state of ease* might be employed to denote the intermediate stages. The ultimate state of ease of course corresponds to the "natural state" of the ideal perfectly elastic solid.

At the point B in Prof. Kennedy's figure we reach the *limit of perfect elasticity*, and enter the stage b of *elastic instability.* Prof. Kennedy's suggestion of "limit of stability" for the point C is inconsistent with the last. I would suggest *elastic crisis* as an alternative for "breaking-down point." We evidently here pass the critical point in the static equilibrium of the molecules.

Perhaps c' might be called the stage of *thermal inversion.*

At C_1 the bar enters the *plastic state*—divided by Prof. Kennedy into the *stage of uniform flow* from C_1 to the point D of *maximum load* and the *stage of local flow* from D to the point E of *terminal load* or (apparently) of *maximum stress.*

(2) I observe that Prof. Kennedy uses "load" and "external stress," apparently as alternative terms, and that Prof. Pearson speaks of "stress per unit area." Would it not be advisable to settle, once for all, that *stress* shall always, when it stands alone, mean a force per unit area? "Stress" and "intensity of stress" would then be identical terms, while the *force* across a given area due to stress would be known as the "total" or "resultant stress" across the area. This is all that is required to bring the terminology of *perfect* elasticity into exact correspondence with that of hydromechanics, in which pressure and total or resultant pressure have always stood in this relation to one another.

(3) Next as to "tension." The word was originally adopted from the theory of strings, and of bars used like strings to support weights, and was, I believe, invariably used (as it still is in the case of strings) to denote the load, or *total* longitudinal stress endured. Nowadays, however, it seems to be employed indifferently in this sense and in that of intensity of tensile stress. I would suggest that the term *traction*, which the modern French writers have freely adopted, should be invariably used to denote intensity of tensile stress, and that *tension* should be restored to its original signification of total or resultant traction.

"Traction" and "pressure" would then (according to the ordinary convention as to sign) be synonymous with "positive" and "negative" stress. Perhaps some elastician would suggest a convenient abbreviation for "total pressure" or "negative tension."

(4) Is it too late to protest on behalf of that much-abused term *viscosity* as applied to solids? The thoroughly-established sense of the word, as applied to fluids, implies—*not* the property in virtue of which they undergo permanent or continued change of shape under continued distorting stress (*i.e.* their *fluidity*); but that other property in virtue of which they are able to offer more or less resistance, by means of molecular friction, to instantaneous changes of shape under stress which is not continued. In this case, therefore, viscosity is a property distinctly opposed to fluidity, and, indeed, described in terms as a falling short of "perfect fluidity."

It is thus obviously false analogy to describe a metal in the state of plastic flow as *viscous*, or to "appropriate this name to that permanent set which may be produced by the application for a long period of a stress well within the limits of elasticity." The latter sense—at least as applied to ice (NATURE, vol. xxxii. p. 16)—has, no doubt, a classical authority in the great memoir of Forbes; but Sir W. Thomson has pointed out ("Enc. Brit.," Art. "Elasticity," § 31; and Thomson and Tait's "Natural Philosophy," § 741) that the properties of ice so described are included under the perfectly definite and convenient term *plasticity*, which is really analogous to fluidity.

On the other hand, analogy demands that the term *viscosity*, as applied to solids, shall be strictly confined to that frictional dissipation of energy which is always at work during rapid changes of strain, and which was first discovered during small vibrations within the elastic limit by Sir W. Thomson (*Proc. Roy. Soc.*, May 18, 1865, or the passages above cited).

That the viscosity of a ductile material is very greatly increased in the plastic stage is of course beyond a doubt, the amount of energy absorbed by it on sudden increase of the stress being so much in excess of that required to provide for the increased potential energy of the accompanying strain that the temperature rises to a sensible extent. But what I wish to make clear is that the true viscosity is not essential to or characteristic of the truly plastic state, but that, on the contrary, the viscosity of a ductile solid renders it *imperfectly plastic* in just the same sense as a viscous fluid is *imperfectly fluid.*

(5) Finally, I may perhaps be permitted to add that, next to the importance to all concerned of a definite and universal terminology, comes the importance to mathematicians at least of a uniform *notation.*

The effect of reading through, for purposes of comparison or historical record, the 100 odd *really important* treatises on this subject—in each of which a perfectly independent and generally

quite different notation is employed—is simply infuriating! I would urge upon Prof. Pearson that he has now an unrivalled opportunity of fixing in the language of English (and perhaps foreign) mathematicians a really serviceable and significant system of notation.

The double-suffix notation for strain and stress, which is developed to perfection in St. Venant's French translation of Clebsch, has many advantages, but seems to be too cumbrous for English taste. Nothing perhaps could be more unmeaning than Thomson and Tait's notation for "stresses," independent as it is of all reference to the strain-symbols. Still I must confess (in common, I dare say, with most men who have derived their first inspirations from that mathematical epic) that it has secured too firm a place in my mental machinery to be lightly cast out, even in favour of a better. W. J. IBBETSON

Cambridge, May 12

The Colours of Arctic and Alpine Animals

MR. R. MELDOLA has maintained, in NATURE, vol. xxxi. p. 505, the idea that the white colour of some animals, Arctic mammals and birds, must be ascribed to the absorbent and radiating power of the same colorations in relation to the rays of the sun. He maintains also that to a similar cause we owe the seasonal polychromism of several mammals and birds of the Alps, and what would be for these animals a partial return to the characters of the Glacial epoch.

By an analogous theory the author explains the contrary phenomenon that is observed in many insects—that is, the darkening of the coloration, and he speaks principally on this point of the Lepidoptera.

Now I beg to make the following observations, and to indicate the following facts:—

(1) That a seasonal mutation of colour is observable in many mammals, now more, now less distinctly, and generally it concurs with the change of coat. Also not seldom in mammals strictly belonging to the Alps, as, for example, in the *Rupicapra europea*, and in the *Capra ibex*, the colour changes very little in the summer and in the winter, although the length, the thickness, and also the coarseness of the hairs were very different. In other cases, as, for example, in the *Cervus mandarinus*,[1] the coat is, in summer, light reddish yellow, with many round white spots, while in winter it is dark brown, and the round spots are less numerous and are light brown.

(2) As to the insects, it is observed that in *Coleoptera* the colours of the Alpine species are brighter than those of the warmer plains, as in the genera of *Carabus*, *Pterostichus*, &c. In several species of *Harpalus*, *Amara*, *Cicindelis*, &c., the individuals that we find at the greatest elevations of the Alps have often lighter colours.

(3) A darker colour and sometimes a whole melanism is observed in general in the insects of the deserts—for example, in that of Sahara. On the contrary, the mammals of these countries present in general a very light colour. It seems to me that this fact cannot be explained by the theory of radiation.

(4) A very remarkable melanism is also observed in several mammals, the Reptilia and Coleoptera that are in little islands, or upon rocks in the warmest regions, for example the *L. muralis*, &c., *Cicindela campestris*, in the island of St. Peter in Sardinia.[2]

(5) In the reptiles and in the Alpine amphibia we sometimes meet with some cases of darkening, but the cases of a remarkable brightening are not very rare, as, for example, in the *tadpoles* of *Rana muta*.

(6) A sensible difference is observed in the coloration between the Arctic birds and the Antarctic. In these last black is much more abundant.

Indeed, Australia, New Zealand, &c., are countries known for a remarkable darkening in the colours of many sorts of animals.

In the Carnivora, which are the mammals that chiefly present seasonal polychromism and white colour, is observed a tendency to this colour in several forms that, however, do not live either in Polar regions or in very cold places. As to this fact the colour of the genera *Zorilla*, *Meles*, &c., and also the very curious *Ailurus melanoleucus* of Thibet,[3] should be observed.

[1] Milne-Edwards, "Recherches pour servir à l'Histoire Naturelle des Mammifères," tav. 22, 22a. Paris: Masson.

[2] Si consulti L. Camerano, "Richercheinterno alla Distribuzione dei Colori nel Regno animale." *Mem. R. Accad. Scienze di Torino.*

[3] Milne-Edwards.

The causes, I would say in conclusion, that intervene to modify the colour of animals, are very complicated; climate has amongst these a certain importance, but it does not seem to me that, although it be very attractive, Mr. Meldola's theory of radiation is sufficient. LORENZO CAMERANO

Zoological Museum of Turin

On Certain Stages of Ocular After-Images

IN a short note in the *Phil. Mag.*, 1872, vol. xliii. p. 343, Prof. C. A. Young has recorded a curious instance of "after-image," which seems to me to be of the same order as that observed by Mr. Shelford Bidwell, and recorded in NATURE, (vol. xxxii. p. 30). I quote from Prof. Young's note, which is named "Note on Recurrent Vision," a few lines, which will show what his observation was:—

"In the course of some experiments with a new double-plate Holtz machine belonging to the College (Dartmouth, America), I have come upon a very curious phenomenon, which I do not remember ever to have seen noticed. The machine gives easily intense Leyden-jar sparks from 7 to 9 inches in length, and of most dazzling brilliance, at the rate of seventy a minute. When, in a darkened room, the eye is screened from the direct light of the spark, the illumination produced is sufficient to render everything in the apartment perfectly visible; and, what is remarkable, every conspicuous object is seen *twice* at least, with an interval of a trifle less than a quarter of a second—the first time vividly, the second time faintly; often it is seen a third, and sometimes (but only with great difficulty) even a fourth time."

Prof. Young shows that it is a subjective phenomenon, and measures the interval between the first and second seeing of an object, giving as the mean of twelve experiments the interval 0·22 second for the case of his own eyes, and 0·24 second for that of another observer.

Five or six years ago I observed another instance of what I believe to be the same kind of "after-image," though at first I was inclined, being engaged upon experiments with a view to finding the cause of certain ocular "ghosts" due to multiple reflection inside the eye (*Proc. Roy. Soc.*, No. 223, 1883), to ascribe it to a different cause. It was seen in a room lighted only by the bright glow of coals in the grate. Whenever the eyes were suddenly flashed across the fireplace, and then fixed on some object 50° or 60° from it, there appeared a faint blue light, which seemed to flash from the object to the glow. This phenomenon was much more strongly marked at some times than others, and varied with some cause which I never further investigated. Later I came upon another instance of the same thing; and as this is the easiest to reproduce, and one by which one may best study the phenomena, I will describe it.

Let a match or a splinter of wood be made to glow, as for testing oxygen, and let it be observed in a dark room; the eyes should be fixed, and the glowing match moved about. I found that for purposes of rough measurement a most convenient curve of motion is a figure of 8 on its side in a vertical plane (∞). Also it is convenient to keep the period of the movement the same, and to vary the size of the curve if change of velocity is required. There are difficulties to be overcome in regulating the brilliancy of the light (Mr. Bidwell has pointed out the necessity of a certain degree of brilliancy in the case of the vacuum tubes), if a systematic investigation were undertaken; a glowing match becomes brighter the quicker the movement; the reverse is the case with a platinum wire carrying a strong current of electricity; and a small incandescent lamp is objectionable on account of reflection from its glass case.

I shall consider the "after-images" of the glowing-point as forming a trail, in which all the changes are set out at the same moment, and proceed to describe the trail for two cases. I should state that following descriptions refer to the trails as seen by me *in the evening*; for there are very considerable variations in the phenomena according as the eye is likely to be wearied or fresh. I may also repeat Mr. Bidwell's caution that it is by no means certain that a person new to the subject will at first be able to see the appearances described.

I arrange a metronome beating seconds, and move the glowing-point so as to describe the curve completely in two seconds. First, let the figure of eight be only as large as can be got into a rectangle 3 inches by $1\frac{1}{2}$. In this case there comes after the glowing-point a dark interval in the trail, about an inch long; then a distinct blue-green ghost, about the same size as the